covery that fermentation, putrefaction and finally, that many diseases are due to micro-organisms, stimulated studies which led to the establishment of the science of bacteriology. The revival in the nineteenth century of the question of spontaneous generation is mentioned, and the great triumph of Pasteur in demonstrating the falsity of the position of the heterogenists. Here also one notes another omission—no reference is made to the luminous researches of Tyndall on this subject with optically pure air. The great influence of embryology as founded on the work of Pander and von Baer is sympathetically although briefly treated. The facts that all animals begin as single cells, and show every gradation between that simple condition and the more complex one of the adult, and that ontogeny is in a sense an epitome of phylogeny, are sufficiently striking to endue this subject with unusual interest. Lastly, the influence of the establishment of the theory of evolution is spoken of.

In physiology the fundamental importance of experiment is pointed out—what the microscope is for anatomy, experiment is for physiology. Among the greatest advances mentioned in the first half of the century are the demonstration of Bell's law and the elaboration of the theory of specific energy by Johannes Müller. The development of physiology along the respective lines of chemical and physical physiology is discussed, together with the opposition aroused by these researches to the old theory of vitalism. The observations as to the action of chemical substances within the bodies of lower animals were turned to practical account in medicine. While physiology was being developed along chemical lines by one school, represented by Claude Bernard Pettinkofer, Voigt, Pflüger, Heidenhain and others, it was being advanced along physical lines by Robert Meyer, Helmholtz, Ludwig, Dubois-Reymond and others. With the latter school came exact methods of measuring and recording physiological activities, as with the kymograph, myograph, etc. The greatest triumph of the chemical and physical methods was in demonstrating that physiological processes are chemico-physical rather than vital. But this conception has been carried too far; some physiologists look upon life, with all its complex manifestations, as being entirely chemical and physical. This is as far wrong as the old theory of vitalism. The relation of the physicist to biological questions is similar to that of the chemist. Physiological questions can not be explained on purely chemical and physical grounds. We can not find out the rôle played by albumin in vital processes by study of its chemistry, but by direct study of the protoplasm in living cells. We must return to an anatomico-biological basis and let it be modified by the chemico-physical conception. The material world must be united by biological studies with the manifestations of the immaterial world of life.

William A. Locy.

Comparative Physiology of the Brain and Comparative Psychology. By Jacques Loeb. The Science Series. New York, G. P. Putnam's Sons. 1900. Pp. x+309. $1.50.

Professor Loeb's book forcibly calls attention to the importance of the comparative method in physiology and psychology. The present work is a translation, with additions and changes, of the German edition of 1900 by Mrs. Loeb. The book has been made into English with singular skill. It is clear, concise, scientifically accurate in statement, and, withal, readable. Of it may truthfully be said 'every words counts.' Whether one agrees or disagrees with any or all of the conclusions reached, the discussion is valuable, for it pleads for opposition, contradiction, investigation. There are not so very many physiologists, we fancy, who will fully agree with all the theories which Professor Loeb seeks to maintain; fewer still are the psychologists who will find themselves in sympathy with his attitude, and among ethical thinkers scarcely any will come to the support of the new scientific construction whose possibility, nay, necessity—for our author is evidently a man of strong convictions—is hinted at. But opposition is needed for the testing of the theories in which the book abounds, although we doubt not that in the main the author's position is a safe one. Nothing is clearer than the seri-

ousness with which Professor Loeb takes himself and his work. Every sentence indicates that he realizes the importance of the study of life phenomena, and appreciates the intimate relationship of all physiological investigation to the practical problems of our work-a-day life.

The book is itself a collection of experimentall discovered facts of neural physiology so arranged that they inevitably lead the reader to the definite conception of the rôle of the nervous system held by the author. Noteworthy is the fact that a majority of the experimental studies from which evidence is drawn in support of the views presented have been made by Professor Loeb himself. They form, as thus gathered together, and unified by a single purpose, a splendid monument to the energy, patience and enthusiasm of a physiologist who has well earned the praises of the scientific world. To say of most physiologists 'he has based a general discussion of the function of the nervous system solely upon the results of his own researches,' would be equivalent to characterizing and condemning the work as narrow and incomplete. But of the work in question this cannot be said, for Professor Loeb's investigations have covered such a wide range of physiological and psychological phenomena, and his problems have been selected with such rare insight into the general implications and relative importance of different aspects of his chosen work, that they furnish an excellent foundation for the theories which he presents.

The conscious and avowed goal of Professor Loeb's researches is 'the control of life phenomena.' His slogan well might be 'more life and fuller that we want.' It is not simply to understand the functions of the organism for the sake or satisfaction of understanding, but that we may be the better able to regulate our lives that we should strive toward the control of vital processes. Toward this goal we are to progress by the use of the methods of physics and chemistry. To quote, "It seems to me that living organisms are machines and that their reactions can only be explained according to the same principles which are used by the physicist." On the basis of the physical and chemical qualities of protoplasm our author proposes to explain all activities.

'Comparative Physiology of the Brain and Comparative Psychology' may for review be divided into four parts. Of these the first deals with the relation of reflex action to the nervous system. Experimental evidence is presented to prove that such actions are not dependent upon any specific character of nerve tissue, but upon the general properties of protoplasm. This is followed by a consideration of the morphological and physiological evidence bearing upon the 'center theory' and the 'segmental theory' of the nervous system, with a defense of the latter. The third main subject is the relation of instinctive action to the rôle of the nervous system and to reflex action. Experiments are cited to show that the instinctive act is really only a chain of reflexes. Finally, in the portion of the book which comes under the title 'comparative psychology,' associative memory is pointed out as the psychic fact of prime importance. Its connection with brain functioning is discussed, and the possibility of analyzing all complex psychic phenomena into associative processes maintained.

It is common to refer all actions to nerve centers. Even the simplest reflex act is thought of by many as dependent upon the functioning of a ganglionic center. Now it is Professor Loeb's conviction that this is an erroneous view; and by an examination of experimental studies of representatives of the Cœlenterata, Echinodermata, Vermes, Arthropoda, Mollusca and Vertebrata he proves that reflexes can be executed in the absence of ganglia. A few of the instances mentioned in the book may be cited.

Study of the Medusæ has shown that the bell will make normal spontaneous, coordinated movements after the nervous system has been removed. Thus by a very simple experiment, spontaneity, coordination and reflex action are proved to be independent of the central nervous system.

Among the Ascidians, *Ciona intestinalis*, whose nervous system consists of a single ganglion, normally exhibits a peculiarly characteristic reflex in the closing of both the oral and aboral openings, when open, if either is

touched. If the ganglion be removed the animal will still carry out this reflex. But careful study shows that there are differences between the action of the normal animal and the one which lacks its ganglion. In the latter the threshold of stimulation is higher and the reaction-time greater. From this Professor Loeb concludes that 'the nerves and ganglion only play the part of a more sensitive and quicker conductor for the stimulus.'

The tentacles of *Metridium* in the food-taking activity of the animal move in such a way as to bring the object with which they are in contact to the mouth. The action is one of apparent adaptation, and one would scarcely expect to see the same kind of a reflex occur after the tentacle had been cut from the body. Such, however, is the result of the experiment; no difference between the action of the tentacle in normal relation to the nervous system and that which has been isolated is observable. We have to conclude, therefore, that the action is determined by the properties of the protoplasm of the tentacle itself, and not by special properties of the nervous tissues.

Other observations prove that certain worms when deprived of their brains are able to move spontaneously. As a case in point, the fresh-water Planarian, *Planaria torva,* is sensitive to stimulation by light. To any increase in intensity it responds by movement. It also selects the darkest region of a dish in which it is left. When the anterior portion of the body is cut off it is found that the brainless portion will give the same responses to light as the normal animal. And the only significant fact in favor of the influence of the brain upon such activities is that the reaction-time of the brainless animal is longer.

Of the reflexes of higher animals that are known to be in part at least independent of the brain and cord are the movements of the iris, the bladder, rectum, blood vessels, respiratory organs, etc. Experiments upon fishes, frogs and dogs have established the independence of many of their reflexes. In one instance the whole brain and spinal cord of a larval frog were destroyed without interfering with spontaneous movements. There are so many observations of this kind for the vertebrates that one cannot question the general truth of Professor Loeb's conclusion. As he states, those instances in which the reflex is interfered with by the destruction of the nervous system are explicable by the fact that the only existing connection between the sense organ and the muscle has been broken. Establish any kind of protoplasmic connection between the region in which the disturbance arises and the motor apparatus, and the appropriate reflex will be executed.

But the fundamental experiment for the proof of the independence of reflexes is that made by Professor Loeb upon isolated muscles. He has shown that muscles containing no ganglion cells will beat rhythmically when placed in a pure sodium chloride solution of the same osmotic pressure as the blood. To put the matter in his words, "It is not the presence or absence of ganglion cells which determines the spontaneous rhythmic contractions, but the presence or absence of certain ions. Na ions start or increase the rate of spontaneous rhythmical contractions; Ca ions diminish the rate or inhibit such contractions altogether." Now it is clear that to prove the isolated muscle capable of responding to stimuli is to establish the thesis which has been stated.

The conception of the nervous system as a series of more or less intimately related centers, each with its own special function, arose and found an observational basis in the many experiments on localization. Certain parts of the brain apparently control certain groups of muscles. This is undoubtedly true, but Professor Loeb attempts to show that the conception as a whole is false. From comparative morphology and physiology comes abundant evidence of the segmental theory of the nervous system. In the Annelids, for example, the segments, each with its ganglion and nerves, are to a certain extent independent organisms. Each ganglion in this case functions in connection with a very definite portion of the worm.

"The so-called centers of the cerebral cortex are merely the places where the fibers from single segments of the central nervous system enter." If the spinal cord of a dog be cut it will be found, after the shock of the operation

has passed, that all of the reflexes belonging to the segments of the body which are represented by the portion of the cord severed can be executed. Rubbing of the skin causes scratching movements of the hind legs, and the reflexes of bladder, rectum and respiratory organs occur in response to the appropriate stimuli.

"According to the segmental theory," writes our author, "there are only indifferent segmental ganglia in the central nervous system, and the different reactions or reflexes are due to the different peripheral organs and the arrangements of the muscles. The center theory must remain satisfied with the mere problem of localizing the apparent 'seat' of a 'function' without being able to give the dynamics of the reactions of an animal, as the latter depend in reality upon the peripheral structures, and not on the structures of the ganglia. For this reason the segmental theory alone will be able to lead to a dynamical conception of the functions of the central nervous system."

In taking up the problem of instinct Professor Loeb touches a field which he has made distinctively his own; for no one has done so much as he toward the analysis and explanation of this type of action. He holds that there can be no sharp separation of reflex action from instinctive. The reflex usually is the activity of a single organ, and the instinctive act one in which the whole organism is concerned. Instinctive actions are characterized by an apparent adaptation to a special purpose, an adaptation to the circumstances in which they occur. For this reason there is a strong tendency to regard them as results of intelligence. For years Professor Loeb has been studying, and, as it were, dissecting, instinct after instinct in order to show the absurdity of this conception.

The fly that instinctively selects for the depositing of its eggs a substance on which the larvæ can feed does so not because it has a faint notion of the utility of the action, or even because it chooses so to do, but because the chemical particles emanating from the substance stimulate it in such fashion as to cause an orientation of its body in reference to the source of the stimulus, and this orientation in turn determines the movement toward the substance. The whole is simply a mechanical problem; physics and chemistry serve to explain the instinct.

An insect comes within the range of vision of a frog and is instinctively stalked, seized and swallowed. In this event the visual stimulus initiates a series of reflexes whose result is the obtaining of food. There is no deliberate choice, no intelligence in the action. The crustacean or insect or worm that instinctively moves toward a source of light does so, experiments indicate, simply because the light forces it to take a certain orientation, just as the chemical stimulus did in case of the fly. Once having taken this position, there is only the possibility of moving toward the source of the stimulus. The 'orientation theory' is one of Professor Loeb's chief contributions to the explanation of instincts. It is based upon the assumption that when a stimulus affects symmetrical points of an animal's body unequally there is resulting inequality of muscular activity on the two sides, and as a result the organism is finally forced into that position in which symmetrical points are equally stimulated. Such a position is evidently attained when the long axis of the body is parallel with the rays of light, for example, with the head either toward or away from the source of the light. From this position it is clear the animal can move only toward or away from the light.

And again it is noticed that certain arthropods and worms 'hide' in crevices. If we study this action we learn that the animals are able to remain quiet only when the body is in contact with some object, and so long as it is not in such a position the animal moves about continuously. In the act 'hiding' plays no part. The phenomenon is merely the inhibition of movement by a stimulus, it matters not whether the stimulus be given by a board which really 'hides' the animal or by a plate of glass which leaves it fully exposed to view. And so one might go on with the enumeration of simple instinctive acts that appear to be guided by reason, but whose careful study reveals only the influence of certain environmental factors upon a definitely describable organic structure.

To the most important of these environmental factors the name 'tropism' has been applied. Heliotropism is the response to light; chemotropism, to chemicals; galvanotropism, to electricity; stereotropism, to contact; geotropism, to gravity; hydrotropism, to moisture; thermotropism, to temperature, etc. All simple instinctive acts are found to be responses to one or more such factors either external or internal; those more complex actions of which nest building and the instinctive processes of ants, bees and wasps are representative are presumably due to a number of factors working simultaneously and giving rise to a series of reflex acts, the whole of which in their interconnection is an instinctive action. No one has yet succeeded in satisfactorily analyzing any of these complex activities, but Professor Loeb has confidence that the subjecting of any of them to laboratory requirements will reveal the same kind of structure as has been discovered in the simple acts.

The chapter on instinct closes with some remarks concerning the relation of the conception presented to ethics. "The analysis of instincts from a purely physiological point of view will ultimately furnish the data for a scientific ethics. Human happiness is based upon the possibility of a natural and harmonious satisfaction of the instincts." Such are the significant statements with which we are introduced to the author's ethical philosophy. From the naturalistic point of view ethics can have no other foundation than that indicated above; and there is no doubt that he who is only a physiologist can find complete satisfaction in it. But one feels, *instinctively,* that Professor Loeb, despite his unpleasant, though appropriate, introductory words concerning the mixing of metaphysical and scientific conceptions, is of a philosophic mind, and it seems probable that physiology alone saved him from becoming a technical metaphysician.

We find upon turning to the discussion of comparative psychology that Professor Loeb considers as the central and chief problem of the physiology of the central nervous system the study of the 'mechanisms which give rise to the so-called pyschic phenomena.' As the elemental psychic fact he names 'associative memory,' by which he means neither more nor less, so far as we can see, than what the psychologist designates as an associative process. Wherever associative memory is found there is material for the psychologist. His first task must be to determine in what animals this psychic phenomenon occurs, and his second, to analyze the more complex processes of higher animals into the elements of the psychic process, much as the instinctive act is analyzed into reflexes.

An animal which can learn is said to have psychic processes. In this criterion of associative memory is seen, by Professor Loeb, the basis of a future comparative psychology. Among vertebrates it is well known that associative processes are found; even the Amphibia and Fishes profit by experience, although it is stated by the author that the frog has not yet been proved to have associative memory. Of the invertebrates in this respect little is known for they have not been studied experimentally. But at present it seems safe to say the Cœlenterata and Vermes are not known to profit by training. By this criterion of the psychic a very sharp limit for the field of psychology is indicated. Those who do not believe in what Professor Loeb describes as crises in development will not be likely to take much stock in his conception of the rôle of comparative psychology until experimentation has proved the abrupt appearance of the associative process in the animal series. For until then there will remain the possibility that the whole thing is a matter of degree of ability to profit by experience, rather than of the presence or absence of a brain mechanism which is able to mediate the association. On this point the author says, "The idea of a steady, continuous development is inconsistent with the general physical qualities of protoplasm or colloidal material. The colloidal substances in our protoplasm possess critical points."

Two chapters of great interest treat of the 'Cerebral Hemisphere and Associative Memory' and 'Anatomical and Psychic Localization.' Concerning the valuable experimental data furnished in them we may make only

a general statement. It proves that the associative process in vertebrates is dependent upon the cerebral hemisphere. "The assumption of 'centers of association,'" says the author, "is just as erroneous as the assumption of a center of coordination in the heart. Association is, like coordination, a dynamical effect determined by the conductivity of the protoplasm. Associative processes occur everywhere in the hemispheres (and possibly in other parts of the brain), just as coordination occurs wherever the connection between two protoplasmic pieces is sufficient. It is just as anthropomorphic to invent special centers of association as to invent special centers of coordination."

Finally, attention should be called to the stress which in this valuable contribution to the literature of comparative physiology is laid upon the chemical and physical study of protoplasm and its transformations. Ultimately it would appear all physiological investigations resolve themselves into problems of the physics of colloidal substances.

In this imperfect and inadequate review of Professor Loeb's book an attempt has been made to indicate a few of the general tendencies and conclusions which seem of prime importance. There are a large number of interesting experimental studies discussed in the book which have not even been mentioned here. We have taken the liberty to quote freely from the text, and it is hoped that the sentences thus selected to indicate the author's point of view will in no case misrepresent him because of their isolation.

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Plant Life of Alabama. An account of the distribution, modes of association and adaptations of the flora of Alabama, together with a systematic catalogue of the plants growing in the State. By CHARLES MOHR, Ph.D. Contributions from the U. S. National Herbarium. VI. Washington. 1901. 8vo. Pp. 921. 12 plates and 1 map.

The 'Plant Life of Alabama' is a noteworthy addition to the list of works which treat of State floras. The book consists of two parts; one, of 127 pages, dealing chiefly with the floristics of the vegetation, the other, of 708 pages, containing a complete catalogue of the flora. The first part will be particularly welcomed by phytogeographers as the first serious analysis of a portion of the vegetative covering of the southeastern United States. The value of this portion lies chiefly in the observations and lists which it contains, as no systematic investigation of the vegetation has yet been made. The absence of recent methods and the lack of detailed formational analysis detract much from this part, though the lapse of time between the completion of the manuscript and its publication would seem to indicate that this is not the fault of the author. It is much to be regretted that the author's death occurred before his book finally appeared.

The author sketches the history of the botanical exploration of Alabama, giving a brief account of the labors of Bartram, Peters, Buckley and others. This is followed by a summary of the general physiographical and climatic features of the State. Physiographically, the area considered falls into five regions, the coastal plain, the region of crystalline rocks, the region of the coal measures, the Coosa Valley and the Tennessee Valley. The author gives a brief discussion of the general principles underlying plant distribution, in which he has unfortunately made use of Merriam's divisions of the North American continent, which are phytogeographically incorrect. The formational treatment is based upon the work of Willkomm and Warming. The accurate classification of formations, however, as hydrophytic, mesophytic or xerophytic, is hardly to be determined otherwise than by actual physiometric investigation of formations, which have been tentatively determined by means of floristic. The formational analysis of the vegetation is neither close nor thorough, consisting for the most part of floristic lists of the various habitats, with very slight consideration of the interrelations of the species which constitute the formation. In some instances (page 65) the difficulty seems to rise from the fact that the acquaintance with the particular vegetation is at second hand.